nature of the controversy in order to counteract the effect of Mr. Spencer's endeavours to represent it as a controversy between those who think that forms of thought become hereditary and those who do not. The original attack centred upon the fallacious character of certain would-be *à priori* proofs of physical laws. Mr. Spencer has tried to parry the attack by maintaining that the writer misunderstood the sense in which the phrase *à priori* was used. That the new interpretation was not the one which it was at the time intended to bear is rendered as clear as the English language permits by his speaking of one of these truths as *not* resulting "from a long registry of experiences gradually organised into an irreversible mode of thought," and his using similar, or even stronger expressions of the others. But this is, after all, not the real issue. No definition of *à priori* would cure the fallacies in the proofs in question or in the subsequent attempts that he has made to support them. They are as illogical with the one definition as with the other; and the sole result of Mr. Spencer's change of front will be, I think, to supply the critics of his writings on Physics with another instance of his habit of changing the meanings of the terms he employs without perceiving that by so doing he forfeits the right to use previous conclusions, even though legitimately obtained, and destroys all connection between the bases and the later parts of his system.

As I have already said, I have been chiefly addressing myself to Mr. Spencer's readers. My aim has been to show that his writings on Physics are marred by superficial and inconsistent views of the subject-matter and fallacies in reasoning thereon. I have been accused of being too violent in my language, and some of my friends have urged, like Mr. Hayward, that it would have been better had I used expressions which less adequately conveyed my (and their) opinion of the magnitude of the errors I was attacking. As I have left the department of personal abuse wholly to Mr. Spencer, I do not think he has much right to complain, even though I have not hesitated to call absurdities by what seemed to me descriptive and suitable titles, and I will conclude this by calling attention to a last effort by Mr. Spencer to show that there is some excuse for expressive language on my part, provided always it is directed to the blunder rather than to the blunderer.

IN NATURE, vol. ix. p. 461, Mr. Spencer asserted that the second law of motion was a mere corollary from the general postulate that cause and effect are necessarily connected together, and in all cases by definite quantitative relations. As every mathematician will at once see that there is a great difference between asserting that there is *some* definite relation between cause and effect and asserting that this relation is the *particular one* of direct proportionality, it will be asked how he came to consider the one a mere deduction from the other? It will be seen, on examination of the passage, that he is misled by a couple of instances that he cites (and of course he might have cited countless others), where there is this simple relation between a prominent part of the cause and a prominent part of the effect. The fallacy of this was pointed out by a writer who signed himself "A Senior Wrangler" in the next number of NATURE, and to this Mr. Spencer replies in the number for May 7:—

"Nor should I care to discuss any question with my new anonymous assailant, who, when certain examples given *show the* '*exact quantitative relations spoken of to be those of direct proportion*,' describes me as 'intensely unmathematical,' because I subsequently use the more general expression as equivalent to the more special—which, *in the case in question*, it is."

Now, in the first place, the phrase "certain examples show," amounts to admitting that the argument is inductive in its nature, which is inconsistent, to say the least, with the professions he makes, for the proof is not only not to be an inductive one, but is to render it clear that no such proof of the matter in question could possibly exist; but this is a trifle to that which follows it. Can anyone avoid admitting that the italicised words leave Mr. Spencer committed to at least one of the following propositions:—

1. That these (and similar) instances establish the proposition that the 'exact quantitative relations' between cause and effect are, *in all cases*, those of direct proportionality.

2. That in a proof (other than an inductive one) you may assume the result during the progress of the argument without invalidating the proof.

The first of these is saved from being pronounced contrary to fact by being discovered, on closer examination, to be meaningless; nothing but the most superficial notions of the meanings of the words cause and effect can prevent its being seen to be unmeaning. The second is too common a logical error to need exposing. What examiner in Euclid has not rejected attempts at the solution of geometrical deductions for this fallacy? If a boy has to prove a triangle to be equilateral, cruel mathematicians do not allow him to assume that it is so in the course of his proof. But Mr. Spencer would take a more lenient view of the matter, and would allow him to use "the more general expression (*i.e.* triangle) as equivalent to the more special (*i.e.* equilateral triangle), which, in *the case in question*, it is."

THE AUTHOR OF THE ARTICLE ON HERBERT SPENCER IN THE BRITISH QUARTERLY REVIEW.

MR. COLLIER, in his anxiety to "transfix" me on one of the horns of a dilemma, has shown himself strangely blind to the fact that he could only do so by thrusting at me through the body of his leader, Mr. Spencer. My wound is consequently but skin deep; but what of Mr. Spencer's?

As I have carefully avoided representing Mr. Spencer otherwise than by quoting his own words, the charge of "misrepresentation" (an ugly word, which, I think, Mr. Collier, on reconsideration, will regret having used) falls to the ground; and if, as Mr. Collier clearly enough shows, there be inconsistency in the phraseology used by Mr. Spencer at different times, the responsibility rests with Mr. Spencer and not with me.

The facts are briefly these:—Mr. Spencer first asserted the Second Law of Motion to be an "immediate *corollary of the preconception*," &c. I criticised the assertion. Mr. Spencer characterised my criticism as a proposal to "exemplify *unconsciously-formed preconceptions*." I did not care for the moment to quarrel with this description lest I should multiply and thus "confuse the issues" between us; and so adopting the phrase under the safeguard (insufficient as it now appears to have been, at least for Mr. Collier) of the usual marks of quotation, I noted what appeared to me an admission, implied in Mr. Spencer's remarks, and important as bearing on the real issue between us, that the Second Law of Motion is a "*consciously-formed hypothesis*." Mr. Collier has done well in calling attention to the discrepancy between the first two phrases italicised. He might also have noted the discrepancy between both of these and the third. But the phrases are Mr. Spencer's; and the only crime to which I can plead guilty is that of not having seen the necessity of more explicitly repudiating Mr. Spencer's characterisation of my criticism, and thus saved Mr. Collier from bringing charges against me of "confusing issues," &c., which I can only transfer to Mr. Spencer.

And now having cleared the path of the personal questions which Mr. Collier has raised, I would appeal to him to obtain for me and other perplexed readers of NATURE an authoritative statement as to what Mr. Spencer's latest views as to the Second Law of Motion are. Does Mr. Spencer regard it as an "unconsciously-formed preconception," or as a "corollary of a preconception," or as a "consciously-formed hypothesis?" Each of these views seems to be deducible from Mr. Spencer's language, but I agree with Mr. Collier that they can hardly be regarded as one and the same thing.

I would also remind Mr. Collier that no answer has yet been given to the difficulties which in my first note I showed to attach to the view of the Second Law of Motion as a "corollary of a particular preconception;" and that, unless Mr. Spencer, or Mr. Collier on his behalf, can show that these difficulties are imaginary, judgment will be recorded against them by default by all readers of NATURE who have had patience to follow the controversy thus far.

ROBT. B. HAYWARD

Harrow, June 6

I OUGHT to thank Mr. Collier for the care with which he has explained his previous letter, but to assure him at the same time that I fully understood it before; his italics have only made plain what was accurately and lucidly expressed before, and have only served to convince me that I thoroughly understand his position, and that it is wholly untenable.

I will make one more effort to show this, by pointing out one of the fallacies in Mr. Collier's last letter. He says "Mr. Spencer alleges that this cognition of proportionality is *à priori*; his opponents affirm that this cognition is *à posteriori*."

The "cognition" spoken of is not one, but two. Mr. Spencer alleges that a conviction of a quantitative relation of some kind between cause and effect, such that the greater cause produces the greater effect, grows in our minds from experiences which are antecedent to reasoning. No one denies it. But to call this a cognition of *proportionality* is so utterly inaccurate an expression as to astound me. And the consequences of the inaccuracy

© 1874 Nature Publishing Group

are immediate and evident; it is believed that special cases of proportionality are involved in the general relation, and hence that Newton's Second Law is an *à priori* cognition.

But the cognition which his opponents affirm is a very different cognition, though this is an odd name to give to a mathematical doctrine. What his opponents affirm is that in certain cases forces measured in a certain way are proportional to their effects measured in a certain way; and by proportional they mean proportional and not something else. They affirm that experiment and observation are necessary to ascertain this proportionality; and that experiment and observation, and the method of verification, furnish overwhelming evidence in favour of the truth ot Newton's laws. Their best proof is the *Nautical Almanac*, to those who can understand it and them.

I believe the *à priori* method to be as utterly barren in the future as it has been in the past. When a new truth has been discovered it is easy to say that it is evident *à priori*. Some day the laws of the actions of molecules and their relations to heat and electricity will be discovered by physicists; but I imagine they will be physicists of the type of Rumford and Faraday and Thomson and Maxwell. Meantime it is open to any *à priori* philosopher to anticipate the future.

And now, as far as I am concerned, this correspondence will cease. Mr. Collier is polite enough to say that my letter would have confirmed Sir W. Hamilton in his conviction that the narrow discipline of mathematics produces an incapacity for general reasoning; and he therefore cannot be anxious to continue a correspondence with one so contemptible, so stupid, and so ignorant as he plainly believes me to be.

A SENIOR WRANGLER

I SHALL be obliged if you will permit me to correct a verbal error, of some importance, in my letter (NATURE, vol. x. p. 84). The words "*finished conception*," in col. 2, line 26, should be "*finished pre-conception*."

J. COLLIER

The Glacial Period

BOTH Mr. Belt and Mr. Bonney, have, I think, missed the one point on which the question under discussion turns. The shell-bearing drift-gravels are *well stratified*. I can speak to those in the neighbourhood of Macclesfield, which run up to 1,100 ft. above the sea, being also very delicately current-laminated. I am puzzled to imagine how this structure could be obtained if the gravels were brought to their present position in the way Mr. Belt supposes; indeed its presence seems to me fatal to his hypothesis. It is not the case moreover that all the shells are smashed and scratched. At Macclesfield most of the shells are broken, as one would expect to be the case if they had been tossed about on a shingle-beach; but entire specimens were not very rare. As for scratches, I never saw one on either the shells or the pebbles of these gravels; in the boulder clay, where the included stones are scratched, scratches are occasionally seen on the shells as well.

A. H. GREEN

Cockermouth, June 6

VENUS'S FLY-TRAP (*Dionæa muscipula*) *

THERE are two ways of studying a plant or an animal. One of these consists in the mere contemplation and description of its external aspects and behaviour. Persons who occupy themselves with this sort of study are commonly called naturalists; for it is by them that by far the greater proportion of the facts we possess relating to natural objects has been gained.

But there is another and a much better sense in which a man may be said to be a naturalist. The true naturalist does not content himself with standing at one side and watching the proceedings of nature as a mere spectator. Animated by that insatiable scientific curiosity from which some shrink, in the fear lest it should carry them too far, while the greater part are indifferent, he occupies his whole life in seeking to lift the veil from all that is hidden in nature and in discovering and exposing the springs of every secret process. His restless spirit cannot content itself with contemplation of the mere external aspects of living beings nor even with the most minute and searching study of the forms and structure of organic life. For even if he begin as a botanist or zoographist, a mere describer of plants or animals, he is forced by the perception of that general adaptation of means to ends and ends to means which he sees everywhere, to become first an anatomist then a physiologist. The study of these external aspects leads him, if possessed of that curiosity which is his characteristic attribute, to study their minute structure, and this, the further he goes into it, stirs up in him the desire to penetrate further into the mysteries of their being. For the delight and interest with which the forms, colours, and structure of animals and plants fill us is derived from the conscious or unconscious perception by our minds of their *adaptation*—their fitness for the place they are intended to occupy. I would go further even than this, and maintain that our artistic perception of beauty in nature is, I believe, in great measure derived from the same source.

But to understand nature in the sense of the naturalist we must know not only those aspects which she is willing to present to us but those she is determined to hide. For this end, when we cannot get at what we want by persuasion, we are often obliged to use compulsion.

It is constantly happening to the naturalist, that he has a process, a contrivance before him, a series of phenomena the connection or evolution of which he cannot understand. He stands at one side and watches and learns but little, for nature refuses to tell *why* she does this, or *how* that. Under these circumstances, which recur, not once in a way, but daily and hourly in the study of plant and animal life, what is he to do? Is it his duty to sit down respectfully and wait, in the hope that what is now difficult and obscure may, by the light thrown upon it from right or left, become more or less clear and intelligible? No. This is not the spirit of the naturalist. If nature conceals the truth, we frankly deny her right to do so, and wrest it from her by force. If circumstances are unfavourable, we alter them to suit our ends. If, as repeatedly happens, a number of antecedents are seen to lead to one event, if a number of apparent causes conspire to one result, we proceed in our investigation by taking away first one, then others of these antecedents, until by a succession of trials (or as they are commonly called experiments) we find the true one, viz. that of which the removal or modification abolishes or alters the event. It is thus, and thus alone, that we compel nature to tell "that wherein her great strength lies."

It is my purpose in this lecture to illustrate to you if I can, by an example, that the systematic application of the method of experiment is the only method by which it is possible to become so acquainted with the forces of nature as eventually to be able to convert them to useful purposes (and this is one, though by no means the highest, end of natural knowledge). More particularly is it true of that branch of natural knowledge which *par excellence* we call physiology, that it is by experiment alone that progress has been or can be made; the whole subject being in its present state but a system of experimental results.

A while ago I applied the term forcible to this method because it is the plan by which, as Bacon said, we torture nature. But let us remember that this is a mere figure of speech. In disciplining nature to our ends, in forcing her to give up her secrets, we use no violence, but utmost gentleness. Plant or animal, to be made to tell its story, must be delicately handled, so delicately that, by association, the very care which the naturalist, for scientific ends, bestows on animals and plants, unavoidably engenders a love for them. However right and necessary it may be that we should to-night destroy and mangle these beautiful leaves for our own pleasure and instruction, let us not do so recklessly, for the life and beauty we destroy we cannot with all our science bring back again or imitate.

The name *Dionæa muscipula* was given to the plant when it was first imported from America. It belongs to the family Droseraceæ, a very natural one, *i.e.* one in

---

* Lecture by Dr. Burdon Sanderson, F.R.S., at the Royal Institution, Friday evening, June 5, 1874.

© 1874 Nature Publishing Group